# Comment on “180 Years of Atmospheric CO2 Gas Analysis by Chemical Methods” by Ernst-Georg Beck

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# COMMENT ON "180 YEARS OF ATMOSPHERIC $CO_2$ GAS ANALYSIS BY CHEMICAL METHODS" BY ERNST-GEORG BECK

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Beck has re-interpreted various 19th and early 20th century chemical $CO_2$ measurements, and derived very far-reaching conclusions. His work, however, contains major flaws, such that the conclusions are wrong, as they are based on poor understanding of the atmosphere.

The concentration of $CO_2$ as measured close to the earth's surface is fully governed by atmospheric mixing, and lack thereof. The two main effects are:

(1) The general build-up of an inversion layer during the night, causing the lowest parts of the atmosphere (some nights lower than 100 meters) to be isolated from the large free troposphere. The normal nocturnal production of $CO_2$ by soil and vegetation mixes only into this small layer, and this leads to highly elevated concentrations of $CO_2$. During the day, the contact between the lower layer and the free troposphere is gradually restored, causing the $CO_2$ concentration to sink towards the free troposphere background concentration. The lowest concentrations of $CO_2$ are generally reached in the local afternoon, when the mixing between lower layer and free troposphere is near to completion. Yet, close to ground level, a distinct difference in concentration will remain, its size depending on weather conditions. At an elevation of only 2 meters, one will never observe background concentration (unless in vast, completely source/sink free areas such as deserts or polar ice caps), but at higher elevations (say >40 meters) one can get close. Still, also at those elevations, one measures a $CO_2$ concentration signal that is far from "Mauna Loa-like". If one uses the daily period between 2–4 PM, however, one gets a reasonable average $CO_2$ concentration and seasonal cycle. At much lower elevations, such as all the measurements used in the article, however, this is doomed to fail.

(2) The difference in atmospheric behaviour in summer and winter. Generally, the process of nocturnal inversion and lack of daily mixing is stronger in winter than in summer. This is the reason why $CO_2$ take-up through photosynthesis is much harder to observe than $CO_2$ production through organic material decay and respiration: During the day in summer the atmosphere tends to be well-mixed, and the $CO_2$ loss to photo-synthesis is diluted in the total atmosphere, whereas during night in winter the decay and respiration only mix into a thin layer of atmosphere and are thus clearly visible as considerable increase of the $CO_2$ concentration.

The effects are respectively called the "diurnal" and the "seasonal" rectifier in the literature. Like a diode, namely, they rectify the observation of the $CO_2$ flux: Sources are well visible, but sinks are much harder to observe. "Simple" pictures like Figure 1 in Beck's paper are therefore misleading: In reality the source and sink effects indicated there are not well visible in the atmosphere, since they are obscured by the variability of the mixing processes. The characteristics above are common knowledge among the scientists monitoring and modelling atmospheric $CO_2$. Apparently, however, it is totally unknown to the author and his supporting group. (Compare for example the clear example of the diurnal rectifier in Figure 8 with the author's comment in the caption).

I suspect they never studied modern real-time continental $CO_2$ registrations. This is a pity, because only a short look at measurements at different altitudes from continental towers such as the Wisconsin tower (NOAA, available on-line), the Hungarian Hegyhatsal tower, the Dutch Cabauw and Lutjewad towers, or even the on-line registrations made by Dutch secondary schools (available, soon in completion, on www.rug.nl/fwn/school-CO2-net), would have shown that the measurements presented in the paper are indeed useless for the purpose the author wants to use them, certainly in the way the author interprets them. If anything, a measurement place close to the sea would be the best try (since nocturnal inversion is much weaker over water), and then selecting only those measurements between 2 PM and 4 PM with wind from the direction of the sea. However, based on the information given in the paper, it is not possible to tell if such potentially useful measurement series do exist. The necessary data to judge, namely measurement height, consecutive length of a record and especially temporal resolution, are lacking in Table 2. In the light of the above, the whole "Discussion and Conclusion" section is invalid, including Figures 11–14. In summary, the paper lacks the very basic knowledge necessary to treat atmospheric $CO_2$ concentration measurements properly. The author even accuses the pioneers Callendar and Keeling of selective data use, errors or even something close to data manipulation, but contrary to the author, Callendar and Keeling took the above into account. This paper, with its principal shortcomings that any knowledgeable reviewer would have noticed, has apparently passed the journal's peer review process, which must worry the journal.

# COMMENT ON "180 YEARS OF ATMOSPHERIC $CO_2$ GAS ANALYSIS BY CHEMICAL METHODS" BY ERNST-GEORG BECK

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In a recent article in this journal (Beck, 2007), Beck questions whether the rise in atmospheric $CO_2$ over the past 50 years is truly unprecedented, citing observations that appear to indicate much higher variability in the 19th and early 20th centuries. Beck furthermore asserts that these earlier data have been discredited merely on the ground that they didn't fit an assumption of a $CO_2$ climate connection, in effect accusing the scientific community of exercising inappropriate bias. If Beck's contentions were true, they would overthrow 50 years of scientific advance and discovery. Unfortunately for Beck—as well as for humanity—the claims don't stand up.

A historic perspective is useful. The modern era of $CO_2$ measurements effectively began with work by C. D. Keeling while he was a postdoc at the California Institute of Technology in the mid 1950's. Here he developed a novel $CO_2$ measuring method based on liquid-nitrogen extraction and applied this to analyze samples along the west coast of North America. Summarizing this work, which predated his landmark measurements at Mauna Loa, he writes (Keeling, 1957):

> "Measurements of the concentration of atmospheric carbon dioxide extend over a period of more than a hundred years. It is characteristic of all the published data that the concentration is not constant even for locations well removed from local sources or acceptors of carbon dioxide. Recent extensive measurements over Scandinavia, reported currently in Tellus, emphasize this variability: observations vary from 280 to 380 parts per million of air. These measurements are in sharp contrast to those obtained in the present study. The total variations at desert and mountain stations near the Pacific coast of North America, 309 to 320 parts per million is nearly an order of magnitude less than for the Scandinavian data. The author is inclined to believe that this small variation is characteristic of a large portion of the earth's atmosphere, since it is relatively easier to explain the large variations in the Scandinavian data as being a result of local or regional factors than to explain in that way the uniformity over more than a thousand miles of latitude and a span of nearly a year, which has been observed near the Pacific coast."

Keeling had discovered what is now known as the atmospheric "background", i.e. the bulk of the atmosphere remote from the land surface in which the $CO_2$ concentration is quite constant. Further measurements by Keeling and colleagues on air sampled from ships, airplanes, and the ground confirmed the relative constancy of this background. Within this background, the $CO_2$ concentration was shown to vary systematically with season and with latitude, with variations of typically 10 ppm or less (Bolin, 1963). After several years of measurements, Keeling also discovered that background $CO_2$ concentrations were increasing systematically year by year, a change that was clearly tied to the large quantities of $CO_2$ emitted each year by fossil-fuel burning (Keeling, 1960).

The concept of the atmospheric background has been backed up by millions of measurements made by a community of hundreds of researchers. In the late 1960s, the concept figured in the establishment of the Background Air Pollution Monitoring Network, which coordinated the observations of atmospheric gases world-wide under the auspices of the World Meteorological Organization and which continues today as part of the Global Atmosphere Watch. The concept can also be understood from first principles based on the fact that the free atmosphere is highly turbulent, thus homogenizing the concentration of long-lived gases like $CO_2$ (Bolin, 1963; Junge, 1963) This homogenization applies to greenhouse and non-greenhouse gases alike.

As Keeling grasped already in 1957—before he had shown that $CO_2$ was increasing—the earlier chemical measurements exhibit far too much geographic and short-term temporal variability to plausibly be representative of the background. The variability of these early measurements must therefore be attributed to "local or regional" factors or poor measurement practice (Keeling, 1998). Beck is therefore wrong when he asserts that the earlier data have been discredited only because they don't fit a preconceived hypothesis of $CO_2$ and climate. In fact, this hypothesis was not widely accepted until the late 1970's (National Research Council, 1979). Instead, the data have been ignored because they cannot be accepted as representative without violating our understanding of how fast the atmosphere mixes.

A small number of the earlier observations may in fact have been done with sufficient attention to sampling and analysis methods. Nevertheless, interest in the early observations waned in the 1980s when it became clear that background concentrations in the past could be established more reliably from air archived in ice cores (Neftel, 1985). Although Beck claims that the earlier data exhibit seasonal variations which correspond to modern observations, this claim is unsubstantiated. The diurnal variability that Beck documents is in fact a smoking gun for data being non-representative of the background.

There is clearly no basis for assuming that meaningful background trends can be extracted by averaging the early data over 11-year intervals, as Beck has done. In effect, Beck has turned back the clock to before 1957, rejecting the notion of an atmospheric background, a concept which has stood the test of 50 years of scientific scrutiny.

It should be added that Beck's analysis also runs afoul of a basic accounting problem. Beck's 11-year averages show large swings, including an increase from 310 to 420 ppm between 1920 and 1945 (Beck's Figure 11). To drive an increase of this

magnitude globally requires the release of 233 billion metric tons of carbon to the atmosphere. The amount is equivalent to more than a third of all the carbon contained in land plants globally. Other $CO_2$ swings noted by Beck require similarly large releases or uptakes. To make a credible case, Beck needed to offer evidence for losses or gains of carbon of this magnitude from somewhere. He offered none.

The Beck article provides an interesting test case for E&E's recently advertised willingness to serve as a forum for "skeptical analyses of global warming" (E&E mission statement, Dec. 2006). The result was the publication of a paper with serious conceptual oversights that would have been spotted by any reasonably qualified reviewer. Is it really the intent of E&E to provide a forum for laundering pseudo-science? I suggest that some clarification or review of the practice is appropriate.

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